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REMARKS ON MR. CARRUTHER'S VIEWS OF PROTOTAXITES.

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In the "Monthly Microscopical Journal" for October, 1872, Mr. Carruthers, of the British Museum, has published a paper in which he endeavours to show that my *Prototaxites Logani*, from the Devonian of Gaspé, is a gigantic seaweed, for which he proposes the generic name *Nematophycus*. Though I saw this article some time ago, other avocations have prevented me from attending to it until now.

The tone and manner of the article, I may say in passing, are unnecessarily offensive; and the author bolsters up his argument by unfair assumptions that I am ignorant of some of the most familiar facts of structural botany, facts which were well known to me while he was yet a school-boy, and which are stated or implied in many of my papers on fossil plants. Possibly, however, Mr. Carruthers is already aware of his bad taste in this matter, and it will be to me a sufficiently ungracious task to expose, as I must do in the interest of truth, the worthlessness of the explanation which he offers of the nature of *Prototaxites*. I shall reply to his objections under the following heads:—(1.) The mode of occurrence of *Prototaxites*. (2.) Its microscopic structure. (3.) Its probable affinities.

1. *Mode of Occurrence*.—This alone should suffice to convince any practical palaeontologist that the plant cannot be a sea-weed. Its large dimensions, one specimen found at Gaspé Bay being three feet in diameter; its sending forth strong lateral branches, and gnarled roots; its occurrence with land plants in beds where there are no marine organisms, and which must have been deposited in water too shallow to render possible the existence of the large oceanic Algae to which Mr. Carruthers likens the plant. These are all conditions requiring us to suppose that the plant grew on the land. Further, the trunks are preserved in sand-

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stone, retaining their rotundity of form even when prostrate; and are thoroughly penetrated with silica except the thin coaly bark. Not only are *Algæ* incapable of occurring in this way, but even the less dense and durable land plants, as *Sigillariae* and *Lepidodendra* are never found thus preserved. Only the extremely durable trunks of coniferous trees are capable of preservation under such circumstances. In the very beds in which these occur, *Lepidodendra*, tree ferns and *Psilophyton*, are flattened into mere coaly films. This absolutely proves, to any one having experience in the mode of occurrence of fossil plants, that here we have to deal with a strong and durable woody plant.

These considerations were dwelt on in my published descriptions of *Prototaxites*, but they naturally have more weight in my judgment than in that of Mr. Carruthers. Geologists and palæontologists will appreciate them.

2. *Microscopic Structure*.—It would be tedious to go into the numerous scarcely relevant points which Mr. Carruthers raises on this subject. I may say in general that his errors arise from neglect to observe that he has to deal not with a recent but a fossil wood, that this wood belongs to a time when very generalized and humble types of gymnosperms existed, and that the affinities of the plant are to be sought with *Taxineæ*, and especially with fossil *Taxineæ*, rather than with ordinary pines.

Mr. C., after describing *Prototaxites* according to his own views of its structure, expresses the opinion that "the merest tyro in histological botany" may see that the plant could not be phænogamous. But if the said tyro will take the trouble to refer to the beautiful memoir on the Devonian of Thuringia, by Richter and Unger,* and to study the figures and descriptions of *Aporoxylon primigenium*,† *Stigmaria annularis*, *Calamopteris debilis*, and *Calamosyrinx Devonicus*, he will find that there are Devonian plants referred by those eminent palæontologists to Gymnosperms and higher Cryptogams, which fall as far short of Mr. Carruthers' standard as *Prototaxites* itself. Nothing can be more fallacious in fossil botany than comparisons which overlook the structures of those primitive palæozoic trees which in so many interesting ways connect our modern gymnosperms with the cryptogams.

* Trans., Vienna Academy, 1856.

† I have elsewhere compared *Aporoxylon* with *Prototaxites*, 'Jour. Geol. Soc.' 1862, p. 306. Report on Devonian plants.

It is scarcely necessary to reply to such a statement as that the fibres of *Prototaxites* have no visible terminations. They are very long, no doubt, and both in this and their lax coherence they conform to the type of the yews. In Mesozoic specimens of *Taxoxylon* which I have now before me, the fibres are nearly as loosely attached and as round in cross section as in *Prototaxites*. In these, as in *Prototaxites*, water-soakage has contributed to make the naturally lax and tough yew-structure less compact, and to produce that appearance of thickness of the walls of the fibres which is so common in fossil woods.

Disks or bordered pores in *Prototaxites* I did not insist on, the appearance being somewhat obscure; but Mr. Carruthers need not taunt me with affirming the existence of such pores in the walls of cells not in contact. Pores, if not bordered pores, may exist on such cells, and the wood cells of *Prototaxites* are in contact in many places, as may easily be seen, and even where they appear separate, this separation may be an effect of partial decay of the tissues.

Mr. Carruthers converts the spiral fibres lining the cells of *Prototaxites* into tubes connecting the cells. This is a question of fact and vision, and I can only say that to me they appear to be solid, highly refracting fibres; and under high powers, precisely similar to those of fossil specimens of *Taxoxylon* from British Columbia, and to those seen in charred slices of modern yews. I may further say that Mr. Carruthers' figure (Plate XXXII.) is in my judgment to a great extent imaginary.

But what of the arrangement of these fibres. It is true that, as I have stated, they appear in some cases to pass from cell to cell, and I hesitated to account for this appearance. Mr. C. might, however, have spared himself the remark that "if Dr. Dawson knew anything whatever about a vegetable cell, and the formation of the spiral fibre in its interior, he would not have written such nonsense"—(a specimen, by the way, of the amenities of British Museum Science, as represented by Mr. C.) The possibilities of such an appearance, as yet, perhaps, unknown in the plant-rooms of the Museum, result from the following considerations: (1.) In more or less crushed fossil plants, it is not unusual to see what are really internal structures appearing to pass beyond the limits of the cell-wall, from the mere overlapping of cells. I have good examples in the Mesozoic *Taxoxylon* already mentioned. (2.) In fossil woods the original cell-wall is

often entirely destroyed, and only the ligneous lining remains, perhaps thickened by incrustation of mineral matter within. In this case the original lining of the cell may seem to be an external structure. I have examples both in Mesozoic conifers and in carboniferous plants. Long soaking in water and decay have thus often made what may have been a lining of wood-cells appear as an intercellular matter, or an external thickening of the walls. (3.) In decayed woods the mycelium of fungi often wanders through the tissues in a manner very perplexing; and I suspect, though I cannot be certain of this, that some fossil woods have been disorganized in this way. At the time when my description was published, I felt uncertain to which of these causes to attribute the peculiar appearance of *Prototaxites*. I have now, from subsequent study of the cretaceous *Taxineæ* of British Columbia,* little hesitation in adopting the first and second explanations, or one of them, as probable.

Mr. Carruthers does not believe in the medullary rays of *Prototaxites*. The evidence of these is the occurrence of regular lenticular spaces in the tangential section, which appear as radiating lines in the transverse section. The tissues have perished; but some tissues must have occupied these spaces; and in fossil woods the medullary rays have often been removed by decay, as one sometimes sees to be the case with modern woods in a partially decayed state. Mr. Carruthers should have been more cautious in this matter, after his rash denial, on similar grounds, of medullary rays in *Sigillaria* and *Stigmaria*, contrary to the testimony of Brongniart, Goeppert, and the writer, and the recent exposure of his error by Professor Williamson. That the wood-cells have been in part crushed into the spaces left by the medullary rays is only a natural consequence of decay. The fact that the medullary rays have decayed, leaving the wood so well preserved, is a strong evidence for the durability of the latter. The approval with which Mr. C. quotes from Mr. Archer, of Dublin, the naïve statement that "the appearance of medullary rays was probably produced by accidental cracks or fissures," would almost seem to imply that neither gentlemen is aware that radiating fissures in decaying exogenous woods are a consequence of the existence of

* Report of Geol. Survey of Canada, now in course of publication. The collections contain wood showing the structure of yew, cypress, oak, birch, and poplar, all from rocks of cretaceous age.

medullary rays, [or that water-soaked wood cannot be cracked in this way.]

Perhaps the grossest of all Mr. Carruthers' histological errors is his affirming that some of my specimens of *Prototaxites* show merely cellular structures, or are, as he says, "made up of spherical cells." Now, I affirm that in all my specimens the distinct fibrous structure of *Prototaxites* occurs, but that in parts of the larger trunks, as is usual with fossil woods, it has been replaced by concretionary structure, or by that pseudo cellular structure which proceeds from the formation of granular crystals of silica in the midst of the tissues. Incredible though it may appear, I know it to be a fact, as all the specimens I gave to Mr. Carruthers had been sliced and studied by myself, that it is this crystalline structure which the botanist of the British Museum mistakes for vegetable cells.* I think it right to state here that I not only gave Mr. C. specimens in these different states of preservation, but that I explained to him their nature and origin.

It is unnecessary to follow further the histological part of the question, as my object is not so much to expose the errors of Mr. Carruthers as to illustrate the true structure of *Prototaxites*.

3. *Affinities*.—In discussing these I must repeat that we must bear in mind with what we have to deal. It is not a modern plant, but a contemporary of that "prototype of gymnosperms" *Aporoxylon*, and similar plants of the Devonian. Further, the comparison should be not with exogens in general, or conifers in general, but with *Taxineæ*, and especially with the more ancient types of these. Still further, it must be made with such wood partly altered by water-soakage and decay and fossilized. These necessary preliminaries to the question appear to have been altogether overlooked by Mr. Carruthers.

My original determination of the probable affinities of *Prototaxites*, as a very elementary type of taxine tree, was based on the habit of growth of the plant—its fibrous structure, its spirally-lined fibres, its medullary rays, its rings of growth, and its coaly bark, along with the durable character of its wood, and its mode of occurrence; and I made reference for comparison to other Devonian woods and to fossil taxine-trees.

* In fossil-woods, the carbonaceous matter, being reduced to a putty mass, sometimes partly becomes moulded on the surfaces of hexagonal or granular crystals, in such a manner as to deceive very readily an observer not aware of this circumstance.

Mr. Carruthers prefers to compare the plant as to *structure* with certain chlorospermous *Algæ*, and as to *size* with certain gigantic *Melanosperms*, not pretended to show similar structure. This is obviously a not very scientific way of establishing affinities. But let us take his grounds separately. He selects the little jointed calcareous sea-weed *Halimeda opuntia*, as an allied structure, and copies from Kutz ing a scarcely accurate figure of the tissue of the plant as seen after the removal of its calcareous matter.* He further gives a defective description of this structure; whether taken from his own observation or from Kutz ing, he does not say. Harvey's description, which I verified several years ago, in an extensive series of examinations of these calcareous *Algæ*, undertaken in consequence of a suggestion that *Eozön* might have been an organism of this nature, is as follows:—“After the calcareous matter of the frond has been removed by acid a spongy vegetable structure remains made up of a plexus of slender longitudinal unicellular filaments constricted at intervals, and at the constrictions emitting a pair of opposite decomound, dichotomous, corymboso-fastigiate horizontal ramelli, whose apices cohere and form a thin epidermal or peripheric stratum of cells.” It will be seen at once that this structure has no resemblance whatever to anything existing in *Prototaxites*, even as interpreted by Mr. C., and without taking into account the fact that *Halimeda opuntia* is a small calcareous sea-weed, divided into flat reniform articulations, to which this structure is obviously suited, as it would be equally obviously unsuited to the requirements of a thick cylindrical trunk, not coated with calcareous matter.

In point of size, on the other hand, Mr. Carruthers adduces the great *Lessonia* of the Antarctic seas, whose structure, however, is not pretended to resemble that of *Prototaxites* except in the vague statement of a pseudo-exogenous growth. *Lessonia* I have not examined, but the horny *Laminariae* of our North American seas have no resemblance in structure to *Prototaxites*.

Nothing further, I think, need be said in reply to Mr. Carruthers' objections; and *Nematophycus* may be allowed to take its place along with a multitude of obsolete fucoids which strew the path of palæontology. As to *Prototaxites*, it is confessedly an obscure and mysterious form, whose affinities are to be dis-

* A more characteristic figure is given in Harvey's “North American *Algæ*.”

cussed with caution, and with a due consideration of its venerable age and state of preservation, and probably great divergence from any of our modern plants; and it is to be hoped that ere long other parts than its trunk may be discovered to throw light on its nature. Until that takes place, the above remarks will be sufficient to define my position in regard to it; and I shall decline any further controversy on the subject until the progress of discovery reveals the foliage or the fruit of this ancient tree, belonging to a type which I believe passed away before even the Carboniferous flora came into existence.